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ABSTRACT

Research in the development of prosocial behavior in children is better done by naturalistic than by experimental methods. The choice of what problems to address in social science research depends upon value commitments. The ultimate criterion for evaluating research in the behavioral sciences must be social relevance and utility. The dominant model in studying prosocial behavior has been that of experimental social psychology. The resulting research has focused more on the experimentally manipulable aspects of situations than on the characteristics of people. Experimental methods have determined what problems are addressed, rather than vice versa. In addition, the variables have been operationally defined in simplistic ways and investigated in artificial situations. This may aid experimentation, but it has frequently produced findings which are trivial or untrue. There are alternative methods, such as Piaget's "methode clinique," which are more appropriate for studying such important questions as the way children conceptualize the situations in which they do or do not behave prosocially. These questions and methods are more suitable for acquiring knowledge which has social relevance and utility. Thus, the balance of research effort must be shifted in the direction of more field and naturalistic studies. (Examples of experimental and naturalistic studies are employed in support of this argument.) (Author/BH)

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Choices, Regrets, and Lousy Models (with Reference to Prosocial Development)[1]

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The title demands an explanation. It was frankly concocted very hastily when Bob McCall phoned me sometime early in the spring to say that he needed the title of this address for the Convention Program and he needed it immediately. But it is not a gimmick title and it undoubtedly telegraphs the message that this address will be in the popular tradition of divisional presidential addresses, a call for agonizing reassessments of a field of study, new perspectives and definitions, changes in goals and directions. And, as is the case with most such addresses, you must expect to hear much that has been said before--and sometimes said better--and much that is controversial.

The parts of the title are interrelated. The choices referred to are researchers' choices of problems to focus on and methods of investigation. As will become clearer, the regrets are many but they boil down to the regret that researchers have not made more substantial contributions to the understanding of the development of prosocial behavior. This is due in large measure to adherence to an unsatisfactory research model which became the major determinant of the choices of problems to be studied.

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[1] Presidential address to Division 7 (Developmental Psychology) presented at the American Psychological Association meetings in San Francisco on August 28, 1977.

In some ways, this paper picks up on Bill Hartup's presidential address to this Division last year. He noted that there is not yet a well-established field of social psychology of childhood although the zeitgeist favors the development of such a field. This seems to me indisputable. And he suggested that the situation might be attributable to the stupidity or the laziness of developmental psychologists or perhaps both. I am not in complete agreement with this diagnosis, although it undoubtedly has some elements of truth. Something else was involved also, a zeitgeist phenomenon or fad in research that proved to be detrimental to progress in this area. And that's what I will be talking about primarily.

What we now have in the area of social development is a vast number of important unresolved problems in search of investigators. There is some real activity in that subsection of the field pertaining to prosocial behavior--the study of the genesis and development of caring, sharing, helping, commitment to others and to humane and democratic behavior. The vast majority of the relevant research publications in this area have appeared within the last decade or fifteen years. Nevertheless, the field is already sufficiently mature to warrant thorough self-examination. Such an examination may have significant implications not only for this field, but for other emerging, or yet to emerge, areas of research in social development.

I recently completed a survey of research on prosocial development. Some of it is excellent and contributes appreciably to our understanding of significant issues. Unfortunately, however, such work represents only a small proportion of the research that has been published. To

anticipate the thesis that will be elaborated below, there has been a great deal of wasted, or practically wasted, effort in research in this area. While there are some notable exceptions, the bulk of it deals with trivial issues and "progress" has largely involved learning more and more about less and less. Meanwhile, many critical problems to which time, thought, and energy could have been profitably devoted have been totally uninvestigated--or at best minimally investigated--and we have essentially no data on a large number of central issues.

What accounts for this situation? Let's review first the choices and decisions and then the related problem of models. Imagine you are approaching the area of prosocial development de novo, as everyone who worked in this area 15 or 20 years ago did. Given the vast number of topics encompassed, how would you choose the problem on which to focus your research; what prosocial dimensions and developmental variables would you investigate? In short, how would you make decisions about the goals and strategies of your research?

You would be hard put to derive hypotheses in advance from existing theories or conceptualizations because the bald fact is that there are none that take account of the vast number of phenomena included under the rubric prosocial behavior. Furthermore, there is practially no probability that such a theory will be formulated in the near future.

I would argue that decisions and choices about research in social development cannot be made "objectively" on the basis of scientific considerations alone. Such choices cannot be value-free, impartial, uninvolved; on the contrary, they must be founded on extra-scientific considerations, specifically on an explicit value framework combined

with moral imagination and vision. Furthermore, despite the diversity of attitudes and beliefs among behavioral scientists, I agree with Morton Deutsch that there is an identifiable, shared value framework to which most behavioral scientists adhere, a commitment to participatory democracy and humane values. The ultimate criterion for evaluating research in the behavioral sciences must be social relevance and utility. This implies that the highest priority should be given to research that has a high potential for yielding information that may be useful in fostering and maintaining patterns of social attitudes and behavior that are highly valued: cooperative and helpful social relationships, concern about others together with willingness to devote considerable effort and energy to promoting their well-being and happiness, to insure that all enjoy basic human dignity, freedom, rights, and opportunities.

To answer socially relevant questions requires the study of real children making real responses in the real world. Prosocial responses are naturally--that is, inherently--complex, and their complexity must be acknowledged and reckoned with in research. This means that experimental techniques and laboratory methods may be appropriate for the solution of some problems but they are probably inappropriate for most.

The nature of the problems with which the investigator of prosocial development deals requires examination of many aspects of the child's psychological space, the assessment of broad and pervasive categories of behavior, and in general, the use of molar constructs. Children's views of what is happening and why they are reacting as they do must somehow be tapped directly. In short, the investigator must confront

critical events, the conditions surrounding them, and the motivations and the phenomenology associated with the performance of prosocial acts. And if an adequate theory of prosocial behavior is ever formulated, it will have to account for naturally-occurring social behavior and be based on observations of such behavior and in-depth study of its correlates.

Obvious as these cautions and considerations may be, they have been "more honored in the breach than in the observance," more frequently disregarded than taken into account. This may be largely attributable to the fact that the primary model for research in this field has been, and is, experimental social psychology. The model involved the experimenter's manipulation of one--or at most a few--social variables at a time, generally in a laboratory, or quasi-laboratory, setting, and the assessment of the effects of this manipulation on a single response, often an isolated bit of behavior.

Keep in mind that until 15 or 20 years ago, there was very little research in prosocial behavior. The real surge of activity in this area began during the halcyon days of experimental social psychology, the period during which it became enshrined. Its prestige inevitably generalized to developmental psychology. In fact, most of the research on prosocial behavior in children represents spin-offs from the investigations of altruism and commitment in adults, particularly experimental studies of these phenomena. And most of the studies of prosocial behavior in adults center on the question "Under what conditions do people act prosocially?" rather than on the question "Who --that is, what kind of person--is likely to act prosocially?"

Given the status of the experimental tradition in psychology, and its unique standing as the method of choice in establishing

cause-effect relationships, it was inevitable that as social psychology grew in scope and prestige following World War II, the application of the experimental method to social psychology was an idea whose time had come. From the point of view of the area of social development, or the social psychology of childhood, it was in many ways a poor idea whose time had come.

Obviously, I would not maintain that the experimental method is inherently a poor one. The experimental social psychology model is indeed useful for the investigation of a number of issues in prosocial development. It is highly appropriate for research on at least one major category of determinants of prosocial behavior, the situational determinants. Children often shift their prosocial responses as situations change. Experiments have elucidated the relationships between such situational factors as transient moods, reinforcements, presence of others, listening to preaching, contact with models, on the one hand, and prosocial responses such as helping and donating on the other. Hundreds of studies of this sort have been published, some of them interesting, many of them presenting only trivial findings. It is extremely difficult to discover a meaningful pattern in the findings of these studies or to discern where they are headed, what they contribute to understanding of prosocial phenomena. After surveying the literature on the effects of situational determinants on children's prosocial behavior, I got the feeling that many experimenters had lost their sense of problem. Questions about the results of one study led to the design of another and new studies were frequently simply minor variations of older ones. To illustrate, a number of experimenters reached the

somewhat evident conclusion that transient moods, including feelings of success and failure, may affect children's altruistic behavior; at least temporarily, boys and girls who feel happy, pleased, or successful more readily assist others, share possessions, and donate more to charity, than those who feel sad, displeased or unsuccessful. One experimenter then asked, "Is this the case if the child's success was achieved in competition with others?" and he conducted an experiment to answer his question. It turns out that under these conditions, children are often convinced that they deserve what they have, and the effects of success on sharing are likely to be attenuated.

In view of the tremendous number of significant problems about prosocial behavior that are as yet unsolved, can the contributions of studies such as this be considered more than trivial? If social utility is the criterion of importance, the answer must be "no." Yet such studies abound, one built up from another. I am reminded of the literary critic who noted that a book he was reviewing "filled some much needed gaps." Maybe the gaps in our knowledge of situational determinants are not needed, but filling them hardly seems to be a high priority item. Perhaps the experimental researchers hope that by some process of aglutination or accretion, a coherent explanatory scheme might emerge from their studies. But this hardly seems likely.

Judging by the sheer number of publications, one might conclude that situational contexts are the most important determinants of prosocial behavior, which is, of course, what some psychologists believe. I regard that conclusion as untenable, however, for I believe the effects of factors residing in people--cognitive and personality factors--and

experiential variables including socialization--are more influential than situational determinants. In fact, however, there are fewer data in the area of prosocial development that support this position than the situational position. But this is probably not a reflection of a scientific truth but of the fact that as long as the experimental social psychology model reigns, there will continue to be more research on situational than on interpersonal or enduring environmental antecedents of social behavior.

This brings us to a specification of the deficiences of experimental social psychology as a model for research in prosocial development, the ways in which this model has been detrimental to progress in this domain of research.

I have already alluded to the first: adherence to that model meant that the choice of the problems for investigation was frequently determined by the criterion of amenability to experimentation rather than of social relevance or significance. It is as though the method to be used governed the choice of problem to be studied rather than the reverse. This imposed crippling restrictions on the scope, coverage, and range of problems investigated and on the definition of prosocial variables. Problems that could not be investigated experimentally were excluded from scientific consideration. Consequently, we have no solid data on such self-evidently significant issues as: peer and community influences on prosocial behavior; underlying motivations for prosocial actions; children's perceptions and conceptualizations of situations that elicit such actions; self-image; the interactions of cognitive and affective factors in eliciting prosocial acts; the development of sociopolitical

attitudes associated with humane behavior. And, of course, there are many others.

Second, it is generally acknowledged that prosocial responses are inherently complex, intricate, and multiply determined, influenced by many interacting factors. But experimental studies are designed to investigate the impacts of only one or very few factors. Experimental designs do not ordinarily permit assessment of the many simultaneous interaction effects that must be elucidated if we are to achieve real understanding of prosocial behavior.

Finally, the use of the experimental social psychology model encourages operational definitions of both dependent and independent variables that are artificial, simplistic, very limited, and basically trivial. This is true even in those cases in which experimenters attempt to tackle socially relevant problems such as determining the impact of modeling or nurturance on proneness to altruistic behavior. The independent variables are ordinarily single events that purport to simulate or duplicate real life circumstances, although actually they seldom do this successfully. Thus, in most studies of imitation and modeling, children observe an adult or child model who was previously a complete stranger on one occasion. This is hardly a satisfactory duplication of what happens in real life situations where the model is ordinarily someone with whom the child has repeated contacts and a meaningful, ongoing relationship. In an experiment on effects of nurturance on prosocial behavior, a research associate, previously unknown to the children, shows interest in the child and smiles at her occasionally; non-nurturance is represented by the adult's matter-of-fact

attitude toward the participant. Analogously, the outcome or dependent variable is typically a single, isolated sample of behavior of unknown validity, that is, its relationship to real life behavior has not been established.

Sometimes the situation in which the outcomes are measured are so contrived that it is impossible to know whether or not children interpret the situation as the experimenter assumes they do. Thus, altruism may be measured in terms of the amount children donate to a fictional charity, or to one they have never heard of, in a situation that doesn't resemble anything they have ever encountered before. Or helping behavior may be assessed by observing whether or not children respond to someone in need of help in an unfamiliar situation that may contain many ambiguous elements. I could go on for paragraphs with this sort of illustration of the trivialization of the prosocial variables being investigated.

The artificiality of these operational definitions severely limits the extent to which experimental findings can be generalized to real life situations. Misleading or unwarranted conclusions may be drawn and become widely accepted, with the result that further, and perhaps better, research on these issues is discouraged or delayed. Let me illustrate this point by citing a couple of examples. One deals with the effectiveness of preaching, possibly a useful technique for raising a child's level of prosocial behavior. (Certainly many parents and teachers try to use this technique in this way.) Widely quoted results of early studies by Bryan and Walbek indicated that preaching had considerably less effect than a model's action on children's

prosocial behavior. In these experiments, the operations defining preaching were mild, low-key statements made by videotaped rather than live models. But more recent work by Rushton on the same problem showed that more direct and extensive statements by a preacher who is regarded as an important person by the children may have powerful and enduring effects. This is especially true if the preaching includes reasons for donating to charity or helping others and if it is aimed directly at the children.

The other example is drawn from research on the basic issue of whether or not there are fundamental prosocial predispositions or propensities, that is, whether or not people are consistent and stable over time in their prosocial tendencies. Not surprisingly, assessments of prosocial behavior in highly contrived situations--tapping restricted, isolated bits of behavior--are not generally significantly correlated with each other or with other indices of prosocial proclivity, such as teacher ratings or peer judgments. This was what Hartshorne and May found in their classic studies of the late 20's and what some more recent studies also suggest. It may be tempting to conclude, as many researchers did, that moral responses are specific habits, situationally determined responses, rather than manifestations of general prosocial tendencies. This conclusion was widely accepted, but it does not seem to be warranted, for substantial recent investigations show that there are in fact some consistent and enduring prosocial predispositions. Intercorrelations among prosocial measures based on naturalistic observations and among global indices (composed of several measures of prosocial behavior) are generally positive and significant. The Whitings

found that frequency of acts of "offering help," "offering support," and "suggesting responsibly" were significantly intercorrelated in six cultures and these activities were negatively related to egoistic behavior.

Further evidence of cross-situational consistency or generality and stability in prosocial behavior over time comes from two current and extensive longitudinal studies. In one, conducted by Jack and Jeanne Block, preschool children at the age of four were rated by their teachers on a number of personality characteristics conceptually linked with prosocial behavior. These same children were observed the next year in a realistic situational test in which they had an opportunity to share prizes they had earned with another child. Those who had been rated high in generosity, helpfulness, cooperativeness, empathy, and considerateness, dependability and responsibility at the earlier period proved to be more generous in the laboratory situation at the later age.

During the first observational period of Diana Baumrind's longitudinal study, nursery school children's nurturance and sympathy to others, thoughtfulness, and their understanding of peers' viewpoints--all were evaluated by means of intensive and extensive naturalistic observations. These characteristics were interrelated in ways that suggest that there is an underlying predisposition to social responsibility and altruism. The same boys and girls were systematically observed again in elementary school five or six years later, and again, socially responsible and altruistic behaviors were associated with each other. Moreover, there was striking consistency or stability over time. The correlations between behavior indicative of social responsibility and

altruism during the nursery school period and comparable behavior five or six years later were $\underline{r}$ = .60 for boys and $\underline{r}$ = .36 for girls.

Clearly, then, when naturalistic and realistic measures of prosocial behavior are used, they prove to be positively intercorrelated. The conclusion that prosocial responses are situation-specific is therefore unwarranted; it is a false conclusion or one that holds only when the measures are artificial, contrived, and unrealistic--as is all too often the case in experimental studies.

In summary, the experimental social psychology model is a poor one to use in the investigation of the development of prosocial behavior because it excludes from consideration many essential issues, it does not readily allow for investigation of complex interaction effects, and it tends to trivialize critical problems by operationally defining constructs and variables in artificial, contrived ways.

It must be admitted that these harsh evaluations of the experimental social psychology model are based on the way the model has usually been used, not on the way the model might be used. A 1973 study by Yarrow, Scott, and Waxler demonstrates that some complex familial antecedents of prosocial behavior can sometimes be successfully approximated in an experiment. In their research on the effects of modeling and nurturance on helping behavior, they made use of a model who was well known to the children in nursery school and she modeled altruistic behavior several times and in several different ways. This is in sharp contrast to the usual procedure in which there is a stranger who acts as a one-shot model; their situation is much more like what the child encounters at home or in school. On the basis of this well designed experiment, the

investigators concluded that the optimal condition for the development of sympathetic, helpful behavior is one in which children observe an adult manifesting altruism in principle and in practice, especially in the context of a well-developed nurturant interaction. These are findings that have been substantiated in other studies in which the dependent and independent variables were evaluated in real-life settings and through naturalistic observations and techniques.

The work of Yarrow et al. suggests that experimental approaches may yield highly relevant information about experiential antecedents of prosocial development under certain specified conditions: when the experimental situations realistically reproduce the events or conditions that occur in nature--by which I mean in settings like home, school, and community--and when the dependent variables are typical of the child's prosocial responses. Unfortunately, these conditions are not easily satisfied. While it may be possible to simulate nurturance and modeling in the home satisfactorily, it would be much more difficult to devise convincing reproductions of parental disciplinary practices, democracy or authority in the home, love withdrawal, consistency of discipline, or maturity demands. We should face the fact that most of the significant antecedent conditions with which we must be concerned if we are to understand prosocial behavior defy experimental investigation and must be confronted directly.

Despite its limitations and deficiencies, it is difficult to break the hold of the experimental social psychology model. Working within the framework of this model is rewarded: the investigator can complete studies rapidly; the results seem straightforward, neat and precise; it is relatively easy to get such studies published; and the

researchers can readily convince themselves that they are making real contributions to scientific knowledge. But a cost-benefit analysis would probably show that, overall, this type of research in prosocial behavior has not really "paid off." Despite huge total expenditures of time and energy, the research has yielded little information of social interest or utility and we have not achieved a deep understanding of the origins and development of prosocial behavior. A moral should be drawn from past experience. We can't undo what's been done, but if we recognize how and why we went wrong, we should be able to avoid our past mistakes and poor judgments in the future. To paraphrase Santayana, those who understand the mistakes of the past may not be doomed to repeat them.

I have no doubt that we could have learned more of significance if we had not somehow become locked into one particular research model. Since prosocial development is a multifaceted phenomenon, different aspects of development may be best researched by different means. If we are to make more progress in research in the future, our research perspectives must be broadened considerably. Other models or research strategies must be used more, supplementing if not supplanting, experimental approaches. Methods must be chosen or invented on the basis of their appropriateness for the problems at hand. What is called for are methods that do not violate the inherent natural complexity of the phenomena or define relevant dimensions in artificial ways.

Of course, not all researchers in this field accept the experimental social psychology model, although most do. For some time there has been a small--but happily vigorous--band of researchers working outside that

model and studying the effects of child-rearing practices or different dimensions of parent-child relationships on prosocial behavior. Included are Hoffman, Baumrind, Rosenhan, myself, and a number of others. These researchers have been more influenced by a range of models of research that have been around for a long time than by the experimental social psychology model: research exemplified by the work of the old Harvard Clinic, socialization studies such as Baldwin's, and Sears, Maccoby and Levin's Patterns of Child Rearing, and the in-depth investigatory techniques of such studies as The Authoritarian Personality. And they use a wide variety of research techniques--observations, interviews, questionnaires, Q-sorts--to evaluate the independent variables and multiple measures, some of them naturalistic, some of them situational tests, to measure prosocial tendencies.

Because the studies of these few researchers are well known and widely cited, you may have the impression that there are many researchers working on these problems. In fact, the number is very small--not much more than a handful; they are greatly outnumbered by researchers working on situational determinants. But their studies have this distinctive characteristic: they confront some fundamental issues directly, using sensible and realistic definitions of the variables being investigated, and meaningful, often multiple or composite, measures of the socialization practices and of prosocial tendencies. The information their studies have yielded is the kind that is most urgently needed and richest in implications, most promising from the points of view of social utility and contribution to deeper understanding of the development of prosocial behavior.

It is surprising that Piaget's highly respected and enormously productive investigatory techniques have not been applied in research on prosocial behavior. By meticulous observations in natural or quasi-natural situations, and by posing salient questions and problems to children directly, Piaget studied the nature of the child's reasoning, thinking, perception, and problem-solving in vivo. The methode clinique and questioning about problem-solution served to elicit the child's own conceptions of the situation and of what he or she was doing. In an analogous way, vitally important information about children's motivations can be procured by skillful interviewing. To date we have practically no information on the motives instigating children's prosocial acts. Children can be asked to explain, in their own terms, why they, or others, perform such acts. Such interviews may be used together with situational tests--for example, questioning a child about why she responded as she did to a cry of distress or to an appeal for donations. A child may be interviewed in naturalistic settings, for example, after assisting a peer who has fallen down on the playground.

Another alternative is to conduct interviews on a more "abstract" level, presenting stories and hypothetical events in which a character has an opportunity to assist another at the cost of some self-sacrifice. The child is asked how the situation should be resolved and why the proposed solution is a good one. This technique enables the investigator to examine in depth the child's own motivations in a wide range of circumstances. Questions must be asked in flexible ways to insure that the child's own motives--not simply knowledge of the norms of what "should" be done--are freely expressed.

Creative researchers in the area of social cognition, such as William Damon and Elliot Turiel, have interviewed children, even very young children, most effectively, presenting them with moral problems or issues of social convention and tapping their thinking about these issues. The information elicited is highly pertinent to understanding children's motivations. For example, Turiel's work shows that advances in children's moral reasoning are linked with intrinsic motivation while decisions about complying with social conventions were more likely to be dictated by extrinsic motives.

Using both naturalistic and interview methods, Eisenberg-Berg and Neal have recently studied the motivations underlying children's spontaneous, naturally occurring prosocial acts. Nursery school children were observed at play and each child who performed a prosocial act--assisted another or shared something--was approached immediately and questioned about his or her reasons for performing the act. This approach to the phenomenology associated with the child's prosocial actions yielded information about motivation that could not be obtained by experimental methods.

Studies relating cognitive functions and/or family interaction to prosocial behavior in naturalistic settings are difficult to do, costly, and extremely time-consuming. Although new methods of statistical analyses are helpful, the multivariate data collected are often difficult to analyze and to integrate. These are some of the reasons there are many more adherents to the experimental model than there are researchers who prefer other investigatory approaches. But judged in terms of the salience, richness and social utility of findings, the payoff from

experimental studies is very small, while the potential payoff from the other kinds of studies may be large.

Meaningful studies of the development of prosocial behavior, like studies of social development generally, must begin where the action is and must concentrate directly on broad general dimensions of behavior and antecedent conditions. If you believe, as I do, that fundamental, constructive changes in individuals and in society can be effected by applying knowledge derived from empirical research, then you will agree that somehow the balance of research effort must be shifted in the direction of more field and naturalistic studies. If we continue on the present path of adding bit by bit to a data explosion--I carefully avoid calling it a knowledge explosion--we will learn very little of social utility. And, at the same time, we will lose sight of the ultimate goal of the behavioral sciences, understanding human behavior in order to improve the human condition and the quality of life.